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Interactive comment on "Mid-Holocene climate change over China: model-data discrepancy" *by* Yating Lin et al.

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We greatly appreciate the constructive comments and suggestions on the previous version of the manuscript from Reviewer #3. We have attempted to address every point raised. The following is the outline of the changes we have made, with reference to the order of the comments made by the referee.

Comments from Reviewer #3:

The manuscript entitled "Mid-Holocene climate change over China: model-data discrepancy" by Lin et al. presented a study on model-data comparison by using the pollen data collection in China and PMIP3 mid-Holocene simulations. From the large discrepancy showed in model-data comparison, both in annual mean, warmest month

and coldest month, they conclude that the major reason that PMIP3 simulations do not agree with data is because the vegetation distribution is not properly represented in climate models, where most models do not include dynamical vegetation and the prescribed MH vegetation map is the same as preindustrial. The MH vegetation issues have been recognized in recent years and many efforts are made to reconstruct a better MH land cover map, this includes the PAGES working group on Landcover6k. Therefore a good vegetation map from China would be expected to contribute to an eventual global land-cover map during the mid-Holocene and benefit the paleoclimate community. However, the current work has a somewhat mislead focus and I have the following major concerns.

General comments from Reviewer #3:

1. The reconstructed mid-Holocene climate in their study is largely depend on the pollen data collection. I am not an expert on pollen data, but I am wondering if all the published data use the same standard on data process. Can they be synthesized by Webb1-7 standard and put together for comparison? I hope a reviewer from pollen community may have some insights on the data process. There are no discussion on the potential uncertainties on collected data, at least one comparison with other proxy data can provide the cross-proxy verification. The authors emphasized three original data but no detailed information, which are important if they are not published. When the significant differences are found in model-data comparisons, the uncertainties from the data should be discussed as well. One can not regard reconstruction is the truth. We need to know how reliable is the reconstructed climate from pollen data, given that the IVF method used to reconstruct the climate is a crude estimate. Otherwise it is dangerous if this paper is published and people take for granted that this is the climate (and vegetation map) in China during mid-Holocene.

RE: Thanks for this very important comment, we will answer it point by point: Firstly, can the pollen records collected from papers be synthesized by Webb 1-7? Yes, we need to do some data processing before we use the collected pollen records to reconstruct the climates. Firstly, the published papers only give the pollen diagram, not the pollen assemblages. So we need to digitize the pollen diagram for obtaining the pollen assemblages, and then use biomization to get the biome scores and biome types. Secondly, for age control, different dating methods are used in the collected pollen records, we use CalPal 2007 (Weninger et al., 2007) to correct 14C age into calendar age so that they can be contrasted each other. For lacustrine records, if the specific carbon pool age is mentioned in the literature, the calendar age is corrected after deducting the carbon pool. Otherwise, the influence of carbon pool is not considered. The age series of records were obtained by linear regression or linear interpolation of adjacent dating data. After these preprocessing, a unified chronological standard for all pollen records is built, and then the classification of age control followed the standard of Webb 1-7.

Secondly, what's the potential uncertainties on data reconstruction? How reliable are the pollen data used in this study? How about the cross-proxy verification? For the reliability of pollen data, we have compared them with the BIOME6000 (Fig. 2, page 50), the match between collected data and the BIOME6000 is more than 90% for both MH and PI. For the potential uncertainties on data reconstruction, IVM is relied on the BIOME4, a global vegetation model. Because of the particularity of vegetation types in the monsoon region of China, the BIOME4 needs further improvement of vegetation simulation accuracy in this area. This possible bias in simulating vegetation will lead to uncertainty in reconstruction. In this version, we added more information in Table S4 in supplementary information, we gave the climate variables reconstructed from IVM at each site. Moreover, we also showed the bias on data reconstruction by giving the median value (for instance, column named MTCO) and values indicating the 5% (MTCO1)-95% (MTCO2) uncertainty bands. For the cross-proxy verification, we compared our reconstruction with previous studies over China based on multiple proxies (including pollen, lake core, palaeosol, ice core, peat and stalagmite). Compared to PI, most reconstructions reproduced a warmer and wetter annual condition during MH (Fig. 1 as below), same as our study. In other word, this discrepancy between

model-data for climate change over China during MH is common and robust in reconstructions derived from different proxies. Our study just reinforces the picture given by the discrepancies between PMIP simulation and pollen data derived from a synthesis of the literature.

Thirdly, about the three original data, thanks for the reminder. They have been published, and we added the information in Table 1, as well as the reference list (line 956-958, page 41 and line 866-869, page 37).

2. The BIOME4 produced vegetation pattern in fig5 is determined by the input climate variables from the model, given the supplementary figures s1-s6 and previous studies by Jiang et al. (2012) have already show different climate patterns produced by different models, therefore the mismatch in vegetation pattern and reconstructed map in Fig5 is expected. I don't think this mismatch can be used to argue that the modelled MH climate is not good because they did not use a correct vegetation map and include the vegetation-climate interaction. Those vegetation patterns produced by BIMOE4 are not used in PMIP experiment setup, it would make more sense if authors compare the reconstruction and PMIP prescribed land cover map, or compare BIOME4 produced vegetation map with the ones produced by those climate models (for example HadGEM2-ES) that have dynamical vegetation to gain some understanding on vegetation-climate feedback.

RE: We totally agree with the reviewer, it's a very efficient way to test our proposal if we can run the simulation with the reconstructed vegetation in GCM. However, as far as we know, prescribing the vegetation in a coupled GCM is not easy. For instance, if we want to use the reconstructed vegetation in Orchidee (the vegetation module of IPSL), we need to modify numerous parameters to make sure that the experiment with new vegetation condition will not be killed by the model due to its mismatch in climate variables. Moreover, the GCM models in PMIP3 have their own vegetation module, it definitely takes much time to do such test, that's why in this paper we choose BIOME4 to evaluate MH vegetation simulation against the reconstructed result. Actually, we already plan to conduct this experiment in Orchidee to further test the proposal of this paper, it's an ongoing work. Although we can't prescribe the MH vegetation with our reconstructed results in all PMIP3 models, we have succeeded to conduct such test in CESM version 1.0.5. The CESM version 1.0.5, developed at the National Center for Atmospheric Research, is a widely used coupled model with dynamic atmosphere (CAM4), land (CLM4), ocean (POP2), and sea-ice (CICE4) components (Gent et al., 2011). Here, we use $\sim 2^{\circ}$ resolution for the CAM4, configured by $\sim 1.9^{\circ}$ (latitude) $\times$ 2.5° (longitude) in the horizontal direction and 26 layers in the vertical direction. The POP2 adopts a finer grid, with a nominal 1° horizontal resolution and 60 layers in the vertical direction. The land and sea-ice components have the same horizontal grids as the atmosphere and ocean components, respectively. Two experiments were conducted, including a mid-Holocene (MH) experiment (6 ka) with original vegetation setting (prescribed as PI vegetation for MH) and a MH experiment with reconstructed vegetation (6 ka_VEG). In detail, experiment 6 ka used the MH orbital parameters (Eccentricity=0.018682; Obliquity=24.105°; Angular precession=0.87°) and modern vegetation (Salzmann et al., 2008). Compared to experiment 6 ka, experiment 6 ka_VEG used our reconstructed vegetation in China. Except for the changed vegetation, all other boundary conditions were kept unchanged in these two experiments, including the solar constant (1365 W $m-2$), modern topography and ice sheet, and pre-industrial greenhouse gases ($CO2$ = 280 ppmv; $CH4$ = 760 ppbv; $N2O$ = 270 ppbv). Experiment 6 ka was initiated from the default pre-industrial simulation and run for 500 model years. Experiment 6 ka_VEG was initiated from model year 301 of experiment 6 ka and run for another 200 model years. We analyzed the computed climatological means of the last 50 model years from each experiment here. The new-added Fig.8 in manuscript (enclosed here as Fig. 2) shows the climate anomalies between two simulations (6 ka_VEG minus 6 ka), for both annual and seasonal scale. For temperature, it's clear that the 6 ka_VEG simulation reproduces the warmer annual ($\sim$0.3 K on average) and winter temperature ($\sim$0.6 K on average), especially the winter temperature. For precipitation, the reconstructed vegetation leads to higher annual and seasonal precipitation,

which can also reconcile the discrepancy of increase amplitude for precipitation during MH between model-data (data reproduced larger amplitude than model, revealed by our study). So it's true that the mismatch between model-data in MH vegetation has significant influence on the discrepancy of climate, this is consistent with our proposal in this study. Each model has different sensitivity to the boundary change, further work should be carried out in more models to test the influence of vegetation on climate, this is an ongoing work.

Specific comments:

1. The abstract need to provide more information from this work, now only contains motivation and conclusion. And the conclusion in abstract actually is a speculation, did not come from the results of this work.

RE: We modified the abstract as following: The mid-Holocene period (MH) has long been an ideal target for the validation of Global Circulation Model (GCM) results against reconstructions gathered in global datasets. These studies aimed to test the GCM sensitivity mainly to the seasonal changes induced by the orbital parameters (precession). Despite widespread agreement between model results and data on the MH climate, some important differences still exist. There is no consensus on the continental size of the MH thermal climate response, which makes regional quantitative reconstruction critical to obtain a comprehensive understanding of the MH climate patterns. Here, we compare the annual and seasonal outputs from the most recent Paleoclimate Modelling Intercomparison Projects Phase 3 (PMIP3) models with an updated synthesis of climate reconstruction over China, including, for the first time, a seasonal cycle of temperature and precipitation. Our results indicate that the main discrepancies between model-data for MH climates are the annual and winter mean temperature. A warmer-than-present climate condition are derived from pollen data for both annual mean temperature ($\sim$0.7 K on average) and winter mean temperature ($\sim$1 K on average), while most of the models provide a linear response driven by the seasonal forcing (a decreased annual mean temperature with a warmer summer and colder winter). By conducting simulations in BIOME4 and CESM, we show that to capture the seasonal pattern reconstructed by data, it is critical to assess surface processes. These results pinpoint the crucial importance of including the non-linear of the surface water and energy balance to vegetation changes.

2. Take line 49 as an example, 0.5K should be write as 0.5 K, follow SI standard, there is a space between number and unit. May correct throughout the manuscript.

RE: Corrected. Line 25, 26, 55, 73, 77, 78, 79, 237, 238, 240, 245, 248, 250, 255, 257, 259, 260, 388.

3. Line 116, "The new sites", if it is new, the data information should be described, otherwise they are unknown.

RE: Corrected by adding the description of new sites "91 digitized data and three original data" on page 6 line 123 in revised version.

4. Line 120, what is cloudiness, how are they measured? Because this is not a common variable, should be described.

RE: "Cloudiness" means the "Total Cloud Fraction", it is calculated for the whole atmospheric column, as seen from the surface or the top of atmosphere. Include both large-scale and convective cloud. The standard output name in PMIP is "clt". It's an inverse measure of sunshine (corrected in page 6 line 128 in revised version).

5. Line 129, how do you determine the anomalies for biome scores? What is the purpose of this paragraph L120-L139, to produce reconstruction in Fig5?

RE: To determine the anomalies for biome scores, we first use the biomization (Prentice et al., 1996) to get the biome score calculated from pollen taxa percentage for both MH and PI. And then we get the biome score anomaly (MH-PI). The purpose of line 120-139 is to demonstrate the scheme of artificial neural network (ANN) used in our study, and by using this interpolation method, we get the reconstructed spatial pattern of vegetation in Fig. 5 with red rectangle. The schematic diagram of ANN is provided as

below (Fig. 3).

6. Line 143 to Line 147, on description of PMIP is a bit strange, what do you mean"in which the PI experiment was denied". "The main variability between MH and PI" should be "The main forcing between MH and PI".

RE:From Line 143 to Line 145, we previously wrote as "In its third phase (PMIP3), the models were identical to those used in the CMIP5 experiments, in which the PI experiment was defined", here we mean that the protocol of PI is defined in CMIP5, and the models of PMIP3 followed that from CMIP5. Now according to the suggestion from you and Patrick Bartlein, we deleted the words "in which the PI experiment was defined". We corrected the "variability" into "forcing" (on page 7 line 156 in revised version).

7. Line 156, "interpolated to a common 2.5 grid", why do you think 2.5 is a common grid, given the pollen data are very local, 2.5 degree grid is too coarse.

RE: The "common" here means "uniform", we have corrected (on page 7 line 166 in revised version) . For the model resolution, yes, our study focus on China area, not globe, but for simulation, 2.5 degree is not very coarse even for local study. Moreover, some global models used in our study have lower than 2.5°*2.5° resolution, like IPSL-CM5A-LR (96*96), it will not make more sense even if we interpolate it into higher resolution.

8. Line 161-162, How do you obtain the sunshine data from observation and model? Should be described more specific.

RE: The sunshine data could be calculated as an inverse measure of cloudiness. Cloudiness means the "total cloud fraction", it's an output of model which named as "clt". We first obtained the "clt" from each model, then calculate the sunshine based on "clt". In the new version of manuscript (on page 8 line 172 in revised version), we described more specific "sunshine percentage (an inverse measure of cloud area fraction)".

9. Line 184, "Weighting the attributes is subjective", will it cause uncertainties?

RE: As we mentioned in the manuscript, weighting the attributes is subjective because there is no obvious theoretical basis for relative significance. The attributes values listed in Table 4 and Table 5 are according to the previous studies (Skyes et al., 1999; Ni et al., 2000). It may cause some bias, however, it is not likely that different ecologists would assign greatly differing values (Skyes et al., 1999).

10. Line 191, from Zhang et al., 2010, the reference can not be found in reference list.

RE: Added in the reference list (on page 40 lines 933-935 in revised version).

11. I am wondering if the warmest month and coldest month changes between MH and PI (and between the models), or always July and January? Give there is a change in seasonality in MH, authors should mention this.

RE: For reconstruction, we obtained the biological climate based on pollen records, and the warmest month and coldest month are not always July and January for both MH and PI. and for models, we calculated the mean temperature for every month, and selected the warmest and coldest one to compare with the reconstruction. So the change in seasonality during MH doesn't influence our comparison for MTWA and MTCO.

12. Line 261, "with a decrease in the northeastern regions", also decrease in east monsoon region at Yangzi river valley.

RE: We have added the words "with a decrease in the northeastern regions and east monsoon region at Yangtze River valley" (on page 12 line 271 in revised version).

13. Line 310-312, "this failure to capture ..", see above general comment 2.

RE: See the answers to comment 2.

14. Line 320, "triggered" is a weird word.

RE: We corrected it into "caused" (on page 14 line 331 in revised version).

15. Fig 7 on feedback discussion, how do you determine the feedbacks from the cloud cover or surface cover? In Line 356 the authors mentioned the "surface albedo and cloud change are calculated . . .", I don't understand why the changes in forcing can be regarded as feedback, physically it is a climate response to forcing.

RE: The albedo feedback is not identical to the changes in forcing, and it doesn't directly response to forcing. In fact, our philosophy to calculate the albedo feedback is: the forcing change during MH (mainly the seasonal solar radiation change) firstly leads to the seasonal climate change, and accordingly, the vegetation type at that period will be different from PI. Then, the changes in vegetation type have a feedback on climate through the albedo variation. This feedback can be calculated by measuring the changes in radiative fluxes at both Earth's surface and at the top of atmosphere. For instance, if the land surface changes from bare soil to forest, the surface albedo will decrease and the net radiation at land surface increase, in this case, the surface albedo has a positive feedback on the net shortwave flux. The detailed information about how to quantify the feedback are shown in Taylor et al., (2007) and Braconnot and Kageyama (2015).

16. Line 733, "Importance" should be "Important".

RE: Corrected.

17. Table 6, should give more information for meteorological data, how long, and give which month is the warmest coldest month, in line 742, should be "warmest month".

RE: Corrected.

18. Line 744, "stand error" means "standard deviation"?

RE: Corrected.

19. Figure 1, should you mark your three original data in this map separately?

RE: The three original data are marked in the Fig. 1 in revised version of manuscript (on page 49) as green circles.

20. Figure 4, the huge annual precipitation anomaly in reconstruction, how reliable is it? I highly suspect it. The unit for precipitation is mm, does it mean annual 240 mm equal 20 mm/month? I suggest you use mm/month to avoid confusion.

RE: Yes, from our reconstruction, the annual precipitation anomaly (MH-PI) is huge. This increase in mean annual precipitation (MAP) is mainly due to the increased intensity of monsoon in eastern area over China, which brings much higher precipitation during summer, and results in an increased MAP. In Table 6, the regression coefficient (R) between the reconstructed modern MAP by inverse vegetation models (IVM) and observed meteorological values is 0.94, which means the MAP reproduced from the IVM is reliable during present day. But it's also true that there are some bias in MAP reconstruction, in Table S4, we give the median value (MAP) and values indicating the 5% (MAP1)-95% (MAP2) uncertainty bands to show the bias in IVM reconstruction.

21. Figure 6, Line 848 to 850, why do you give the abbreviation, they are not in the figure.

RE: Corrected (page 54 in revised version).

The revised version of manuscript and supplementary information are enclosed as below as supplement.zip.

Reference: Weninger, B., Jöris, O., Danzeglocke, U., 2007. CalPal-2007. Cologne Radiocarbon Calibration and Palaeoclimate Research Package. http://www.calpal.de/. Jiang, D., Lang, X., Tian, Z., and Wang, T.: Considerable Model–Data Mismatch in Temperature over China during the Mid-Holocene: Results of PMIP Simulations, Journal of Climate, 25, 4135-4153, 2012. Gent, P.R., Danabasoglu, G., Donner, L.J., Holland, M.M., Hunke, E.C., Jayne, S.R., Lawrence, D.M., Neale, R.B., Rasch, P.J.,

Vertenstein, M., Worley, P.H., Yang, Z., and Zhang, M.: The community climate system model version 4, Journal of Climate, 24, 4973-4991, 2011. Salzmann, U., Haywood, A.M., Lunt, D.J., Valdes, P.J., and Hill, D.J.: A new global biome reconstruction and data-model comparison for the middle Pliocene, Global Ecology and Biogeography, 17, 432-447, 2008. Prentice, I. C., Guiot, J., Huntley, B., Jolly, D., and Cheddadi, R.: Reconstructing biomes from palaeoecological data: A general method and its application to European pollen data at 0 and 6 ka, Climate Dynamics, 12, 185-194, 1996. Sykes, M.T., Prentice, I.C., and Laarif, F.: Quantifying the impact of global climate change on potential natural vegetation, Climatic Change, 41, 37–52, 1999. Ni, J., Sykes, M. T., Prentice, I. C., and Cramer, W.: Modelling the vegetation of China using the process-based equilibrium terrestrial biosphere model BIOME3, Global Ecology and Biogeography, 9, 463-479, 2000. Taylor, K.E., Crucifix, M., Braconnot, P., Hewitt, C. D., Doutriaux. C., Broccoli, A. J., Mitchell, J. F. B., Webb, M. J.: Estimating shortwave radiative forcing and response in climate models, J. Clim, 20, 2530-2543, 2007. Braconnot, P., and Kageyama, M.: Shortwave forcing and feedbacks in Last Glacial Maximum and Mid-Holocene PMIP3 simulations, Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 373, 2054-2060, 2015.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2018-145/cp-2018-145-AC4-supplement.zip

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Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2018-145, 2018.

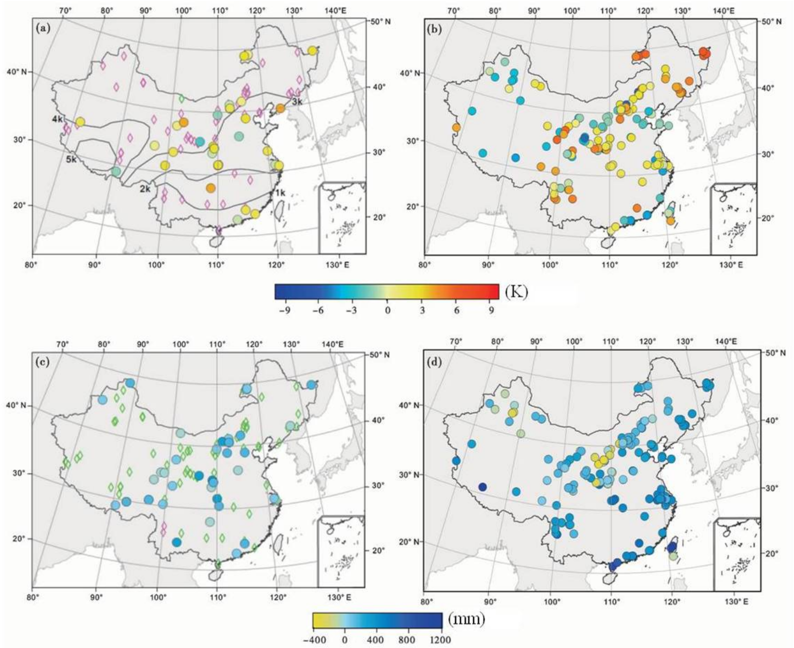

Figure 1. Comparison between our climate reconstruction and previous reconstruction. (a) Previous temperature results. Diamond is the qualitative reconstruction, red is the temperature increase and green is the temperature decrease; Circle is quantitative reconstruction; (b) Mean annual temperature reconstruction in this study; (c) Previous precipitation results, diamond is the qualitative reconstruction, red is the precipitation increase and green is the precipitation decrease; Circle is quantitative reconstruction; (d) Mean annual precipitation reconstruction in this study.

Fig. 1.

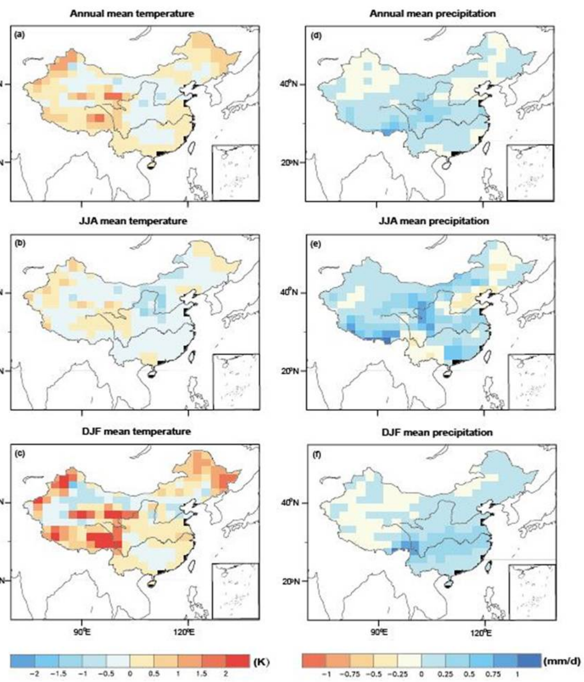

Figure 1. Climate anomalies between the two experiments (6 ka and 6 ka_VEG) conducted in CESM version 1.0.5. The anomalies (6 ka_VEG-6 ka) of temperature and precipitation at both annual and seasonal scale are presented, and all these climate variables are calculated as the last 50-year means from two simulations.

Fig. 2.

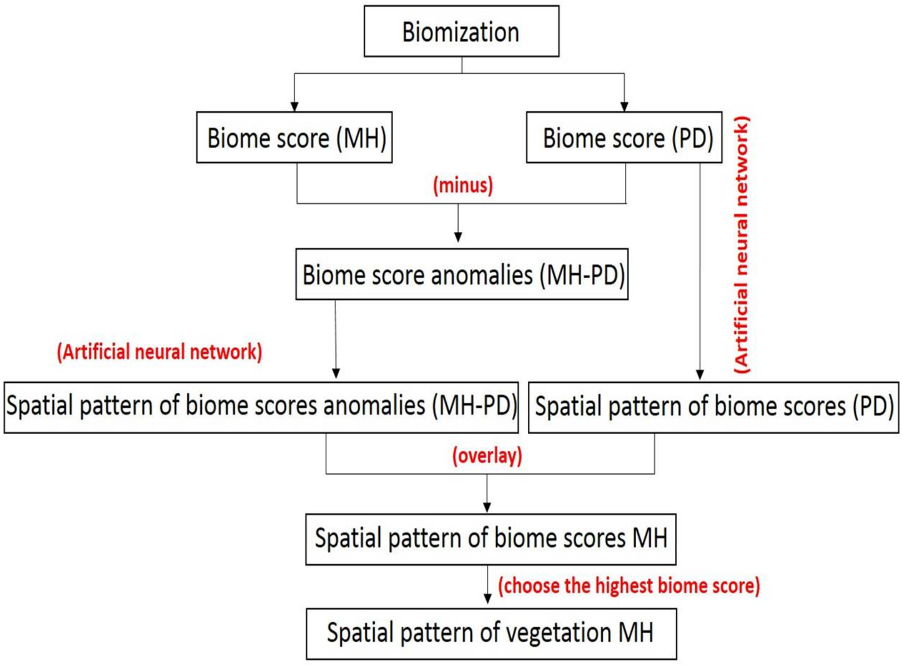

Figure 3. The schematic diagram of artificial neural network.

Fig. 3.